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LETTERS TO THE EDITOR.

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The Subtropical Zones of High Barometric Pressure.

THERE is an old theory, if a mere popular notion which has no scientific basis whatever may be so called, that the two zones of high barometric pressure, extending with a few interruptions around the globe, and having their maxima of pressure about the parallel of 35° in the northern hemisphere, and 30° in the southern, are caused by the crowding of the air, in its passage in the upper part of the atmosphere from the equatorial to the polar regions, into intermeridian spaces, becoming gradually narrower toward the poles. It is supposed that the air, as it is forced into narrower spaces, is turned down toward the earth's surface, and that this descent of the air causes increased pressure on the surface. The barometric pressure in both hemispheres increases from the poles, or at least from some high parallel, toward the equator, until the parallels above mentioned are reached, and then there is a small decrease of pressure to the equator; so that these parallels are simply the limits between the increasing and decreasing pressure gradients in going from the pole to the equator, and the culminating parallels of the convexity of the isobaric surfaces.

The writer's attention was first directed to this feature of these isobaric surfaces about thirty-five years ago, in reading Lieut. Maury's "Physical Geography of the Sea;" and, having no faith in the popular explanation, he made it a matter of study in order to discover the true cause. This was found in the now well-known law of the deflecting force of the earth's rotation, which was first discovered at that time. By this law the air, in moving from west to east in the middle and higher latitudes, is pressed toward the equator; but, in moving the contrary way in the lower latitudes, it is pressed a little toward the poles, thus causing a bulging-up of the isobaric surfaces with the culminating lines between the two systems of easterly and westerly currents about the parallels of 35° or 30°. The results were published in an "Essay on the Winds and the Currents of the Ocean," which was subsequently republished in "Professional Paper of the Signal Service," No. XII.

Subsequently this whole subject was treated in a more thorough and mathematical manner, and the results were published in a memoir entitled "Motions of Fluids and Solids Relative to the Earth's Surface." This was afterwards republished in "Professional Paper of the Signal Service," No. VIII., with extensive notes by Professor Frank Waldo. In this memoir it was shown that with certain assumed values for the velocities of the easterly and westerly motions of the air, which were quite reasonable and probable from what was known of these somewhat uncertain data, the deflecting force of the earth's rotation would give the observed increase of pressure, on the one hand from the pole, and on the other from the equator; so that there was no room to doubt that the maximum pressure a little above the tropics in each hemisphere was caused by this force. A very full abstract of this memoir was also given in *Silliman's Journal*, January, 1861.

Subsequently this same subject was taken up again, and treated in a more thorough manner and with better data, and the results published in "Meteorological Researches," Part I., "Coast Survey Report for 1875."

The same subject was again treated by the use of mathematical processes somewhat simplified, and given in "Recent Advances in Meteorology," forming Part II. of the "Report of the Chief Signal Officer for 1885."

Finally the whole matter was gone over again by the writer in a popular manner, and explained by means of various simple illustrations, and was given in his "Popular Treatise on the Winds," etc.

Dr. Hann, however, has not accepted the results, nor has he ever attempted to show that they have been deduced from erroneous principles or processes, but has continued to use and uphold the old theory. Not only this, but he has based upon it a new theory with regard to the cause of the high-pressure areas of the middle and higher latitudes. In the *Zeitschrift für Meteorologie* for 1879, p. 39, he first suggests that these regions of high barometric pressure may be simply the places where the upper equatorial and westerly currents settle down towards the earth's surface, as in the case of the zones of high pressure at the polar limits of the trade-winds. His idea is, that as the upper poleward-moving currents in the latter are deflected down by their being crowded between intermeridional spaces, gradually becoming narrower toward the poles, so, even beyond these belts of high pressure, there must be local hinderances, or a damming-up of these currents, by which they pass into descending ones toward the earth, and so cause the high-pressure areas.

In the next volume of the *Zeitschrift* he again refers to this matter, and suggests that the reason why cyclones and great barometric disturbances are more frequent in winter than in summer is that in winter the temperature and pressure gradients of the upper strata of the atmosphere, in a poleward direction, are greater, and hence there is a greater strength of current at this season of the year.

Again, in his "Climatology," published a few years ago, this same old theory is given in explanation of the subtropical zones of high pressure.

Finally, in his recent memoir published by the Royal Academy of Sciences of Vienna, the old theory of the subtropical high-pressure belts is introduced, and also his new theory, deduced from it, of the causes of high-pressure areas; and he refers to his preceding papers in the *Zeitschrift* on these subjects.

Although the teaching of Dr. Hann on these subjects has been entirely at variance with the writer's own views on the same subjects, previously published at so many different times, yet he has

refrained from taking any notice of it. But now that this last memoir has recently been brought to the attention of English, and especially of home, readers, justice to himself requires that this matter shall not be allowed to remain unnoticed any longer.

The question of the cause of the high pressure in the subtropical zones, according to the old theory, is one of the relation between kinetic and potential energy; that is, between velocity and pressure. As the air of the upper part of the atmosphere moves toward the poles, it is supposed to become crowded and checked in its motion, and the kinetic energy changed to pressure. But the question arises as to why this takes place up to a certain latitude only, that of maximum pressure, and not all the way up to the poles; for the maximum velocities of the upper poleward-moving currents must be a little above this latitude, and the converging of the meridians increases up to the pole. As long as kinetic energy is changed to pressure, this must be increased; and so the greatest pressure must be at the pole, and not down at a low latitude. But it may be shown that the whole effect is so extremely small, that it is not worthy of any consideration practically.

The following general expression of the relation between pressure and velocity is taken from "Recent Advances in Meteorology," p. 194:—

$$(1) \quad \log P_0 - \log P = \frac{h}{18401\,(1 + .004\tau)} + \frac{s^2 - s_0^2}{360940\,(1 + .004\tau)},$$

in which P is the barometric pressure in millimetres of any part of the air with corresponding velocity s; P_0 equals 760 millimetres, being taken as the value of P at the earth's surface, and the corresponding value of s equals s_0; h is the difference of altitudes corresponding to P_0 and P; and τ is the temperature by the Centigrade scale. If u, v, and x are the meridional, longitudinal, and vertical velocities respectively at any given point, we have

$$(2) \qquad s^2 = u^2 + v^2 + x^2.$$

The numerical constants in (1) are adapted to common logarithms, and the expression is strictly applicable to the case only in which τ is constant and in which friction may be neglected.

The first term in the second member of (1), depending upon h, arises from gravity. Where only small portions of air are considered, or strata of very small depths, the part of the pressure depending upon h is so small in comparison with the whole atmospheric pressure, that it may be neglected, and the expression may then be put into the following form:—

$$(3) \qquad P_0 - P = \frac{s^2 - s_0^2}{206\,(1 + .004\tau)}.$$

This is substantially the same, in different measures and notation, as that of Kaemtz (*Lehrbuch der Meteorologie*, vol. i. p. 150), when used at the earth's surface, where $p' = 760$ millimetres.

In the application of the preceding expressions it is necessary to know the value of s_0 corresponding to P_0; but this is known in a few special cases only, since we do not have a complete solution of the dynamic problem of the general circulation, in which the condition of continuity and the frictional conditions are taken accurately into account. It is also necessary to know the stream-lines, since P and P_0 must be in the same stream-line.

It is evident from the observations of the cirrus clouds at Zi-ki-wei (latitude 31° 12′ north) that the velocity of the poleward-moving current of the upper part of the atmosphere at this latitude cannot be more than about two metres per second, or four miles and a half per hour (see *Popular Treatise on the Winds*, etc., p. 122). Let us now suppose that there is a perpendicular wall on the parallel of 35° extending all around the globe, and reaching up to the top of the atmosphere, and that the whole upper half of the atmosphere has a motion, from some cause, directly against this wall, with a velocity u. The current in this case will pass directly down to the earth's surface, where, near the wall, we must have sensibly $s_0 = 0$. Supposing, now, that $P_0 = 760$ millimetres when the whole atmosphere has no meridional component of velocity, and that $\triangle P_0$ is the effect of the upper current: we get from (1), in this case,

$$(4) \qquad \log (760 + \triangle P_0) = \log 760 + \frac{u^2}{360940\,(1 + .004\tau)}.$$

Putting $u = 2$, and $\tau = 0$, this gives $\triangle P_0 = .0194$ millimetres, or about .00076 of an inch of barometric pressure. The increase of barometric pressure in the high-pressure belt, above the normal pressure, is about 0.3 of an inch. So the old theory, even upon the extreme supposition that the whole kinetic energy of the upper current is converted into pressure in the high-pressure belt, accounts for only about the $\frac{1}{400}$ part of the observed increase of pressure in this belt. When we consider, then, how small a part of the kinetic energy of the upper current is changed to pressure, and that the most of it passes on to higher latitudes, how extremely small must we suppose the effect from the old theory to be!

Where there is friction, of course some of the kinetic energy is changed into heat, and so the pressure is accordingly diminished; and a little greater velocity would be required to cause the same increase of pressure.

In what precedes we have supposed the kinetic energy to have its origin from some other source than a pressure gradient; but in the interchanging motions between the equatorial and the polar regions, toward the pole above, and the contrary below, this is not the case, but the pressure must decrease from the equator to some middle latitude where the velocity u and kinetic energy are the greatest, and then increase from that to the pole, where it is 0 and the pressure the greatest. The preceding formula is applicable in this case at the equator and the poles, since $s_0 = 0$; and, putting $u = 2$ metres per second, we get $\triangle P_0 = .0194$ millimetres, as before. If we suppose P_0 to be in the latitude where $u_0 = u$, that is, where the velocity of the return current is the same as the maximum velocity u above, then, instead of u^2 in (4), we have $u^2 - u_0^2 = 0$, and hence we get $\triangle P_0$ in this case equal 0; that is, there is no change of pressure here arising from the interchanging motion between the equator and the pole. The pressure, therefore, is a little greater at the equator and the poles than at the latitude where u is a maximum, which, on account of the convergency of the meridians, and the narrowing of the intermeridional spaces, toward the poles, is between the middle latitude and the equator, and perhaps near the parallel of 35°. Instead, therefore, of an excess of barometric pressure here of about 0.3 of an inch, there should be a very slight depression, if there were no other forces to cause this excess. And this is very evident from a very simple manner of considering the matter: for as long as the air, in moving from the equator, is acquiring increased velocity, there must be a descending pressure gradient; but, as soon as there is a decrease of velocity, there must be an ascending gradient to cause it. The same is true in the lower strata of the atmosphere, where the air returns from the polar to the equatorial regions. The oscillations of the air-particles between these regions are similar to those of a pendulum, in which the force from both sides acts in the direction of the middle point.

With regard to the effect of descending currents, to which Dr. Hann ascribes the local high barometric pressures of the middle and higher latitudes, already referred to, the formula (4) can be applied in this case also. We have only to substitute for u the vertical component of velocity x. This being done, we can readily compute what the value of x must be to give $\triangle P_0$ equal to any assignable value. Let us suppose it is required to find what value x must have to give $\triangle P_0 = 25$ millimetres; that is, an increase of barometric pressure of about one inch. We can, in this case, assume $s_0 = 0$, at least in the middle of high-pressure area. The formula in this case gives $x = 71.2$ metres per second, or about 160 miles per hour, if we put $\tau = 0$ in the formula. For a higher temperature this velocity must be greater.

If any one is disposed to doubt this result given by the formula, let him take the experimental result obtained by Mr. Dines and others, that a velocity of about seventeen miles per hour gives a pressure of one pound per square foot upon a plate exposed at right angles to the current. But the pressure of the whole atmosphere, corresponding to 30 inches of mercury, is about 2,100 pounds. The pressure corresponding to one inch, therefore, is 70 pounds. As the pressure is as the square of the velocity, we must have $x = 17 \times \sqrt{70} = 142$ miles per hour, to give a pressure equal to one inch of barometric pressure. This result is less than that obtained theoretically, because it is well known that the experimental pressure upon a small plate is greater than the theoretical, on account of the effect of friction of the air which passes around

the plate, both upon the air which is retarded and stopped in front of the plate, and also upon that behind the plate.

It is doubtful whether a descending current in the open air of more than two metres per second could be found anywhere in the whole atmosphere. This, we have seen, would increase the barometric pressure 0.0194 of a millimetre, a quantity which could not be detected by the most delicate and accurate barometer. It is seen, therefore, how very improbable is Dr. Hann's theory of the cause of high-pressure areas.

Dr. Hann lays great stress upon the efficiency of the steep gradients of the upper part of the atmosphere, in the middle and higher latitudes, in producing both cyclones and high-pressure areas. But the forces arising from these gradients are almost completely counteracted by the deflecting forces of the earth's rotation in connection with the eastwardly moving currents in these latitudes, the velocities of which increase with increase of altitude very nearly in the same proportion as the steepness of the gradients Although the steepness of these gradients at high altitudes, especially in the southern hemisphere, is considerable when considered with reference to gravity simply, yet, if all the forces are taken into account, there is no part of the atmosphere in the middle latitudes where the gradients are smaller, the velocity of the easterly motion being such as to not quite counteract the force from the gradients, and to leave a residual force simply which is sufficient to counteract the frictional resistance in these high altitudes, which is very small. It would be just as reasonable to maintain that there is a strong tendency in the water of the ocean to rush toward the poles, because there are steep gradients, considered with reference to the earth's attraction only, and leaving out of consideration that the centrifugal force arising from the earth's rotation counteracts this tendency, as to maintain that the air in these high altitudes has a strong tendency to rush toward the poles. WM. FERREL.

Martinsburg, W.Va., Dec. 22.

Recent Investigation on the Causes of Cyclones and Anticyclones.

IF I were required to name the man who impressed me as the most profound meteorological writer whom I had read, I should without hesitation say Professor Ferrel.

The most of us are qualitative meteorologists: he may be called a quantitative meteorologist. Not content with mere general statements of causes and forces, he attempts to determine the exact value of each one, and by rigid mathematical formulæ to determine if they are sufficient to account for the given results.

This represents a high, if not the highest, development of a scientific mind. For this reason I would hesitate to dissent from Professor Ferrel's conclusions more than from any writer I know; but he has himself, in his recent letter to *Science*, severely criticised the supposed blind following of authority, and, if there were needed any excuse, I would give this as the reason for presenting the views opposed to those of Ferrel.

There are two methods of arriving at results. The one is by deduction, in which the thinker, starting from axioms, well determined constants, or general laws, works out the results which must follow. The other is by induction, in which the thinker starts from observation, or separate individual facts, and arrives at general laws. Both methods are necessary; and most thinkers of to-day will admit that no theory of natural phenomena is complete until the results of deductive reasoning correspond to the results of inductive reasoning, or *vice versa*.

Now, Ferrel is essentially a deductive reasoner. It is necessary in such reasoning that the fundamentals, or physical constants from which one starts, should be correctly determined. In Ferrel's and Marvin's replies to Hazen in *Science* and in the *American Meteorological Journal*, I believe it has been shown that the constants forming the basis of the calculation in Ferrel's condensation theory of cyclones were satisfactorily determined. Starting with these, and following Espy, he has shown, that, given a warmer body of air, or a rapid vertical decrease of temperature over a considerable area, the causes are adequate to initiate and maintain a cyclone.

The question now is, do the investigations of inductive meteorologists sustain these views?

In order to study the results which follow rapid vertical decrease of temperature in the atmosphere, Loomis "selected from the volumes of the published observations of the Signal Service (November, 1873, to January, 1875, and from January, 1877, to May, 1877) all of the cases in which the temperature at Pike's Peak was 40° lower than at Denver." With this difference between them, the air would theoretically be in unstable equilibrium. "The number of these cases in twenty months of observation was 343. Only 39 of these cases occurred during the seven winter months of observation, and they occurred most frequently during the months of May. . . . The facts appear to show that at the dates given there were seldom any extraordinary disturbances on Pike's Peak. In two cases hail was reported, in four cases sleet and in fifteen cases either rain or snow. These facts seem to indicate an occasional uprising, but it is remarkable that so few such cases occurred; and it will be noticed that a difference of temperature of at least 45° between Pike's Peak and Denver often continued from day to day for long periods. . . . I think we may hence infer that dry air, even when greatly heated, has but little ascensional force" (Loomis's "Contributions to Meteorology," 13th paper, in *American Journal of Arts and Sciences*).

Loomis also found that heavy rainfall was not necessarily productive of cyclones. In his sixth paper, after examining a large number of cases, he says, "We conclude, therefore, that great rainfalls do not generally continue over eight hours, and very rarely do they continue for twenty-four hours, either as experienced at one station, or in succession at different places." He arrives at the same conclusion in his seventh and seventeenth papers, and adds, "The forces which impart that movement to the air which is requisite to an abundant precipitation of vapor, instead of deriving increased force from a great fall of rain, rapidly expend themselves, and become exhausted."

Furthermore, after examining a large number of areas of low barometric pressure with which there was little or no rain, he says, "There seems to be no room for doubt that barometric minima sometimes form with little or no rain, and continue without any considerable rain for eight hours, and sometimes for twenty-four hours or longer; . . . so that it seems safe to conclude that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion."

"In order to determine the circumstances under which storms originate and ultimately acquire their full intensity," Loomis selected thirty-six cases from the Signal Service weather-maps in which the storm appeared to develop in the United States, and, as a result of a study of these, says, "The first stage in the development of each of these storms was an area several hundred miles in diameter, over which the height of the barometer differed but little from thirty inches, with an area of high barometer both on the east and west sides, and at a distance of about 1,000 miles. In the few cases in which a high barometer is not reported on both sides of the origin, it is because the area of observation is not sufficiently extended. The mean value of the barometer on the east side was 30.42 inches, and the mean distance 1.033 miles; on the west side the values were 30.31 inches and 977 miles. . . . On Hoffmeyer's storm-charts we frequently find three areas of high barometer surrounding an area of low barometer. These areas of high barometer are regarded as one of the causes, and generally the most important cause, of the storm which succeeds. . . . Since the air presses in on all sides towards this area of low barometer, the area tends to assume an oval form, which may become sensibly circular if the winds are very violent, and the centrifugal force resulting from this revolving motion causes a still further reduction of the barometer. . . . Rain is one of the circumstances which increases the force of a storm, and it invariably attends storms when they have attained considerable violence. . . . Some rain was invariably reported whenever the barometer fell below 29.4 inches, and generally there was some rain reported whenever the barometer fell below 29.5 inches. I have found no storm of great violence which was